Art. XVIII.—*Report of the Gravity Survey Committee of the Royal Society of Victoria.*

[Read 16th November, 1893.]

Your Committee has much pleasure in reporting that good progress has been made in the work of the Survey during the past year. The apparatus sent out from the Kew Observatory by the Royal Society of London has been erected at the Melbourne Observatory in an underground apartment, under the supervision of Mr. Ellery; the observing telescope described in the last report was completed and placed in position last autumn: and an elaborate series of observations taken of the pendulums by Mr. Baracchi, in order to connect Melbourne with the Kew and Greenwich base stations.* Mr. Love has obtained practice in observing, and is now engaged on a series of observations intended partly to supplement those of Mr. Baracchi, partly to form a starting point for the Australasian Survey.

In the course of the work a number of—as the observers think—radical defects in Kater's design for an invariable pendulum have manifested themselves (see appendix). It is hoped that some of these disadvantages will be overcome in the new type of pendulum now under construction by Mr. Ellery at the Melbourne Observatory.

The Committee has expended £12 6s. of its grant, partly cost of package and transport of the pendulums, partly in necessary mechanical work upon them.

Your Committee respectfully asks for re-appointment, with the addition of the name of Mr. Baracchi; and that the unexpended balance of the grant of £25 be placed at its disposal.

E. F. J. Love, *Hon. Sec.*

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* Proc. Roy. Soc. Victoria, vol. vi., p. 162.

Appendix A.

On the design of Pendulum Apparatus for Differential Observations of Gravity. By E. F. J. Love, M.A.

The work of the Gravity Survey Committee, of which I have the honour to be secretary, has forced upon my attention the subject of the various theoretical points which come up for consideration in the designing of pendulums for use in differential observations of gravity. A good many different forms of pendulum are now employed for this purpose by observers in different parts of the world : and it seems not altogether without interest to examine the various types in use, with the view of seeing to what extent they agree with what we may look upon as the ideal pendulum, and, where they differ, to indicate the manner in which improvements might be effected.

(a) The first point to which attention should be directed is the fact that the only measurement which has to be made on pendulums for differential work is the determination of the vibration number in different parts of the earth. This at once places the possible accuracy of the work on an altogether different level from that obtainable in any absolute measurement of gravity : in absolute measurement we are definitely limited by the degree of accuracy obtained in the measurement of the length of the pendulum, an operation very inferior in this respect to the determination of a vibration number by the method of conicidences. But the whole value of the work depends on the extent to which the vibration number of the differential pendulum at any one place maintains its constancy ; in other words, *on the degree of invariability of the pendulum.*

It might seem superfluous to insist on the necessity of so constructing a differential pendulum that it shall be, as far as the nature of material structures will allow, invariable in form and dimensions, but such is not the case. Some observers have conceived the idea that it would be convenient to be able to make both differential and absolute measurements with the same pendulum : notably Commandant Defforges,* who has con-

* Comptes-Rendus des séances de la Commission Permanente de l'Association Géodésigne Internationale, 1888. Annexe vb. p. 12, *egg.*

structed for use in the French geodetic surveys an instrument which he terms a "reversible invertible pendulum:" this instrument has two fixed knife-edges, and two interchangeable weights, which are transferred from end to end of the pendulum in each observation. Commandant Defforges is of opinion that such a mode of construction will do away with the errors inseparable from the use of so-called invariable pendulums. I cannot agree with him here: for it seems to me very doubtful whether we should ever get the weights after displacement back to exactly their original positions: and the errors thus commissible, though small, are quite likely to be of a higher order of magnitude than those which the method is designed to eliminate. For instance, suppose a fine dust particle should get lodged between the weight and its seat: this might well be the ten-thousandth of an inch thick, and yet escape observation: yet it would alter the vibration number in a half-seconds pendulum by one and a half vibrations per day. Moreover, the large amount of extra handling, screwing up, etc., involved seems to me to militate seriously against the trustworthiness of the arrangement. I have entered into this matter at considerable length because Commandant Defforges is looked upon, and rightly so, as one of the foremost of living geodesists, and his work demands very careful consideration.

Positing then the necessity for invariability, as far as it can be attained, let us see how we are to seek it.

I am strongly of opinion that the pendulum should be rigid; this, indeed, was the first point forced on my attention when handling the Indian pendulums. However much care be taken, a five-foot bar of thin brass, with comparatively heavy weights at its ends, is continually undergoing flexures of a rather alarming magnitude during the processes of handling which necessarily go on in the course of the transfer of the pendulum from its case to the cylinder in which it is vibrated; I have serious doubts whether any amount of care can prevent such flexures as will introduce a permanent set and alter the vibration number. Nor are these doubts without solid foundation; we know that these pendulums have been bent in the past, and have had to be re-straightened—a process which necessarily destroys the continuity of the observations made with them—and one of them is certainly not quite straight now. On these grounds I should view with suspicion any design which included a flexible bar; among these may be mentioned the pendulums lately employed by Professor Mendenhall,* which were of the same general pattern as the Indian pendulums, though much shorter, and consequently less liable to injury. The best pattern I have seen is that adopted by Lieut.-Col. von Sterneck, which consists of a rigid rod, carrying at its lower end a bob composed of two frustra of cones united at the base.

Recent practice inclines more and more, and rightly so, to the use of half-second pendulums; for a pendulum of one-fourth the length is, at least, ten times as free from risk of injury in handling. A five-foot bar is distinctly awkward, but a thirteen-inch bar is just about as comfortable a size to handle as could well be imagined, to say nothing of the increased rigidity. This method secures the further advantage of portability, a matter of great importance in survey work.

Not merely should the pendulum be short and rigid, its structure should be, as far as possible, continuous throughout. This, unfortunately, cannot be fully attained; the best which can be done is to have the different parts so made that they can be attached together by a process of shrinking on and riveting. Solder is always objectionable, and its use can be avoided by employing a suitable design.

One great obstacle to the attainment of invariability is the old practice of attaching the knife-edges to the pendulum and swinging it on a plane suspension; for the knife-edges get dulled and have to be reground; each regrinding of course changes the effective length of the pendulum, and destroys the differentiality of the observations. Professor Mendenhall† took a great step in advance when he attached the planes to the pendulum, and swung it on a knife-edge suspension: for a good agate plane requires only to be kept clean; while the knife-edges, if separate from the pendulum, may be reground at pleasure without sacrifice of differentiality. Incidentally this mode of suspension introduces a good many other improvements, which may as well be summarised here.

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* U.S. Coast and Geodetic Survey Report for 1891, appendix 15, p. 503.

† *L.c.*, p. 530.

1st. The difficulty of setting the knife-edges accurately in line is entirely got rid of; for they can be attached, once for all, to their seats in the suspending apparatus, and ground up together *in situ*.

2nd. The difficulty, never yet overcome, of setting the knife-edges accurately perpendicular to the axis of the pendulum is done away with: for the setting of the agate planes can be tested by optical means to any required order of accuracy, and the head to which the planes are attached can be altered till they attain their proper positions. This fact removes the only possible advantage of a flexible pendulum bar. Kater's object in making the bar flexible was to ensure its verticality even if the knife-edges were not quite accurately set; this advantage has always seemed to me rather a doubtful one,* even with Kater's original pattern, but whether it be so or no, the attachment of the agate planes to the pendulum makes any flexibility in the bar quite superfluous.

(b) Not merely should the pendulum be invariable, its shape is a matter of importance; for it must be so designed as to render the resistance of the air to its motion a minimun. This is a serious objection to a form which would otherwise be highly advantageous; Captain Basevi† suggested that a rigid cylindrical rod with a spherical bob would be a good form, because for it, alone of all known figures, the pressure connection could be determined directly by calculation; unfortunately this form offers a great deal of air resistance, and such a pendulum would have its oscillations damped too rapidly for convenient observing. A lenticular figure is probably the best, but it is difficult to make symmetrical; and on the whole, the most convenient seems to be the double-cone pattern of von Sterneck, which can be figured in the lathe with all desired accuracy.

Another point in connection with the shape is the position to be assigned to the pendulum bob. The usual practice is to set its broadest plane vertical and in the plane of swing; but von Sterneck puts it horizontal. I believe this was done by him for constructional reasons, but a further advantage is incidentally secured in this way. It is well known that a flat body moving in a fluid

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Sir G. G. Stokes however thinks differently. *Vide* appendix B.

† G. T. Survey of India, vol. v., p. 92.

tends to set itself broadside on to the direction of motion; if then the bob be vertical and its broadest plane parallel to the plane of motion, it is always trying to rotate about its vertical axis, and in this way either the knife-edges will wabble on the suspension, or (if the pendulum be too heavy for that) a torsional stress is applied to the knife-edge, the direction of the stress being reversed twice in each oscillation: of course this stress is only small, but just as constant dropping wears away stones, so a constantly reversed stress wears out a knife-edge. The case is very different if the broadest plane of the bob be horizontal; here the only effect is a bending stress on the pendulum rod, a stress too small to produce any effect on a rod of ordinary dimensions. If we bear in mind that von Sterneck's rod is about $\frac{2}{5}$ of an inch thick, but that a decent steel knife-edge is not $\frac{1}{100,000}$ of an inch across, the advantage of transferring the stress from the knife-edge to the rod is at once evident.

Another point to be attended to is the position at which the starting lever gives its impulse to the rod. If this be not—as it generally is not—at or near the centre of oscillation, the operation of starting tends to produce a sideways shift of the head of the pendulum, and so to bend over and dull the knife-edge. Now to determine the dimensions of a pendulum of given form which shall have its centre of oscillation at a given point is a matter for calculation; the calculations are rather complicated, but quite manageable by known mathematical methods, so need not be detailed here.

(c) The supports of the pendulum must of course be as frictionless as possible. This is secured by attending to the construction of the knife-edges and planes of suspension; and here we have the advantage of the experience gained by the manufacturers of chemical balances, among whom the general consensus of opinion seems to be that a steel knife-edge and agate planes affords less friction than any other combination. Agate knife-edges cannot be given so fine an edge as steel, and the similarity of the material of planes and knife-edges is a further objection, and although there is of course no risk of rust, there is some danger of splitting the knife-edge; the advantage here is on the side of the steel, for a rusty steel knife-edge can be reground, but a split agate is of no further use and must be replaced by a new one.

(d) However carefully we construct our pendulums we shall always have to correct the observed vibration number for temperature and air pressure, and the corrections are by no means small. Temperature changes affect both the dimensions of the pendulum and the general properties of the medium in which it swings, while pressure affects only the second of these. We therefore require to determine both these quantities with considerable accuracy; for pressure this is easy enough, a good syphon barometer gauge being all that is wanted; but the determination of the temperature is not so simple, and observers are by no means agreed as to the best method of attaining it. The general assumption appears to be that a thermometer will follow the changes of temperature of the air more quickly than will the pendulum. Accordingly many observers, including Sabine and Mendenhall, sink the thermometer bulbs in a metal bar of the same thickness as the pendulum rod, while von Sterneck encloses the whole thermometer in a wide glass tube. The latter plan is almost certainly bad; for the heat has first to make its way through a glass tube, then across a layer of air, and then to heat up the thermometer. But is Sabine's plan very much better? If we consider the structure of a thermometer bulb, viz.: a thin layer of glass, which is notoriously a bad conductor of heat, and then a cylindrical mass of mercury about as thick as, and a worse conductor than, the pendulum rod, and if we bear in mind that convection currents probably play only a secondary part in equalising the temperature of different parts of the thermometer bulb, and further that all delicate thermometers are sluggish in their indications, I think we shall see that an unprotected thermometer with a tolerably large bulb, set as near as possible to the pendulum, will probably lag in temperature behind the air of the containing vessel by about the same amount as the pendulum itself, and in consequence the thermometer is more likely to give the actual temperature of the pendulum if arranged in this way than if sunk in a metal bar or otherwise modified. In any case delicate thermometers are required; they should register at least to one-twentieth of a degree Fahrenheit, preferably to one-fiftieth of a degree Centigrade.

(e) But little need be said as to the containing apparatus. It should certainly be of metal, in order as far as possible to secure

uniformity of temperature throughout the enclosure, and should be painted of a light colour externally, so as to diminish the rate of absorption of heat: it cannot very well be left bright, as the light reflected from it would be a serious obstacle to observing. The apparatus should of course be rigid and tolerably heavy, otherwise the pendulum when swinging will set it in vibration; it should also be a form of very stable equilibrium, else external disturbance may shake it and interefere with the motion of the pendulum. A truncated cone of thick brass seems to answer every purpose; glass windows can be introduced where necessary, and the case should be mounted on *large* levelling screws, which stand in metal grooves on a heavy support of stone or timber. Such an apparatus need not weigh more than half-a-hundred-weight or so all told, and need take up but little room; a striking contrast to the Indian apparatus, which is extremely bulky and weighs nearly a ton.

Appendix B.

Part of a Letter from Sir G. G. Stokes, Bart., P.R.S., *to the Secretary.*

7 Queen's Parade, Bath,
6th August, 1891.

Dear Mr. Love,

You do not say expressly, but I take for granted that in the contemplated gravity survey you mean to use invariable pendulums, not Kater's pendulum, or some other form available for absolute determinations. It is generally, I think, allowed that for determining the *variation* of gravity from place to place the results obtained by invariable pendulums are the more accurate. The series of determinations would be rendered absolute by transporting the pendulums to some station where gravity has been well determined absolutely and swinging them there. It will suffice if the station last mentioned be one for which gravity is accurately known absolutely by comparison, by means of invariable pendulums used by previous observers, with some other station where gravity had been determined absolutely.

At least two pendulums just like each other should be used, in order that any accidental derangement of a pendulum may be detected. Sabine said to me in conversation that there ought to be three, as that would enable you, in the event of any derangement taking place in course of transit or handling, to tell which pendulum it was that had got altered. If you had only two, and one got slightly deranged, you could only tell which it was by going back to one of the stations where they had been previously used and swinging them afresh. However, I think two only have as a rule been all that have been used in gravity surveys, and I believe that with care in packing, transporting, and handling, such derangements are not likely to occur.

Before fixing on the form we must answer the question, Is the correction for the resistance of the air going to be determined by calculation or by experiment?

(a) If by calculation, we are restricted to forms for which it is possible to effect the calculation. The pendulum might be a plain cylindrical rod, or such a rod with a sphere at the end. In an invariable pendulum, soundness of casting would not be of any very great moment, the observations being strictly differential. If a rod be used, I should prefer the ends being made hemispherical, or thereabouts. The exact form is of no particular consequence, for for a small portion of the rod near the end the calculation cannot be effected, whether the rod be left plain, or formed into a hemisphere. The calculation for a sphere would not apply to a hemisphere joined on to a cylinder. But the part of the resistance which depends on what is near the end of the rod forms only a small fraction of the whole, and if we are obliged to have recourse to estimation for that small portion, the uncertainity thence arising can be only very small, since the rod is supposed to be but narrow for its length. The alternative is to adopt the form mentioned by General Walker, a cylindrical rod with a sphere at the end. I do not think there is much to choose between these two forms. I think the latter would keep up its oscillations somewhat longer, and the former would have to be about five feet long (for a seconds' pendulum) which might perhaps be a little inconveniently long. I do not know however that this would be any serious inconvenience.

As to the calculation it is to be remarked that the numerical value of the index of friction given in my paper is much too low. This arises in great measure from my having corrected for the residual air in Baily's swings at reduced pressure (about one inch of mercury) on the supposition (which seemed to be conformable to the single experiment that Sabine had made on the subject) that the coefficient of viscosity, the μ of my paper, varies as the density. We know that Maxwell's law, according to which it is independent of the density, is very accurately true in experiment. The true coefficient is now well known for air. I have not got here books of reference, but towards the end of a paper of Tomlinson's in the Phil. Trans., in which he treats of the viscosity of air, you will find collected the numerical results of various observers, himself included. The effect of reducing, in my paper, by a law as to the relation between viscosity and density now known not to be the law of nature was to exaggerate the effect of reduction of pressure, in other words to under-estimate the effect of the residual air, and therefore, in equating the theoretical expression for the difference between thirty inches pressure and one inch in the observed result, to bring out a coefficient which was decidedly too small. The adoption however of the true law, though it raises considerably the coefficient of viscosity as got from Baily's experiments, leaves it still too small. I do not see how to account for this except on the supposition that the motion of the pendulums was not small enough to allow of a strict application of the formulas of my paper. I have remarked in my paper (at least with reference to a suspending wire, and the same would of course be true generally) that the effect of the formation of eddies would be to tend to throw the effect of the resistance from off the time on to the arc. Whether any sensible part of the resistance is due to the formation of eddies, may be tested by seeing whether the arc of vibration decreases strictly in geometric progression as the time increases in arithmetic. I examined in this way some of Sabine's experiments in the Phil. Trans., and some of Bessel's experiments with the long and short pendulums. Plotting a curve with the time and log-arc for co-odinates, it came a straight line for the long pendulum, but the curve, though very nearly a straight line for the shorter pendulums, had a sensible though slight curvature. It appears therefore that with the amplitude of vibration usual in pendulum experiments, at least in the early portion of the swing the effect of eddies is not wholly insensible, and therefore it may well be that the formula in which the motion is assumed to be small enough to be regular may not be quite applicable to the actual experiments. However, beyond the discrepancy between the calculated and observed decrement of the arc of vibration, which I have mentioned in my paper, and also the decrement being not quite strictly in geometric progression, there was nothing to indicate that the formulas were in any way in fault, so very good seemed the agreement between theory and observation, until it was shown that the correction of the assumed law as to the relation between the viscosity and the density still left the numerical value of the index of friction as determined from the pendulum experiments slightly too small. But in merely differential observations, such as those carried on with invariable pendulums, I think any uncertainty of this kind would be quite insensible provided that care were taken that the observations should be strictly differential or very nearly so. Hence, if you wish to connect a group of Australian stations with Indian stations it is a perfectly open question whether you shall choose a pressure of say twenty-eight inches for the Australian set or a pressure of say four inches (or whatever the usual Indian pressure for India was). That is on the supposition, which I gather from your letter is intended, that you mean to construct new pendulums. The pendulums being different, the two series cannot be connected till the new pendulums are swung at one of the old stations, unless you are ready to trust to a reference of each series to an absolute determination belonging to it. But in either case if the higher pressure were thought the more convenient for the Australian stations, and it were not wished to trust to a correction for so great a difference of pressure as twenty-eight and four inches, it would merely be requisite to swing the invariable pendulums twice in succession at the reference station, once at the higher pressure, to connect with the Australian series, and once at a low pressure to connect with the Indian series or with the absolute determination as the case may be. If the vacuum apparatus be not quite staunch, as I fear may prove to be the case, it might be better, as a matter of convenience and indeed accuracy, and as Colonel Herschel has proposed, to use the vacuum apparatus only for ensuring a constant pressure of say twenty-seven or twenty-eight inches, except of course for the one set of swings at low pressure taken at the station of reference. However, much would depend on the condition of the vacuum apparatus.

I will mention here lest I should forget it that it is well to allow an observation (whether by a single swing, as may be done in vacuo or by a succession of swings does not much matter) to extend over twenty-four hours, or if that be inconvenient at least from dark to dark, through day or night as may be chosen, so as to rate the clock by transits for the interval of time over which the observations extend. For you cannot trust a clock, even though the rate from day to day be very uniform, to be quite exempt from a diurnal inequality of rate.

(b) Suppose now that we prefer to depend on experiment for the correction for the air. Then we may choose our form of pendulum as we please. That usually employed has the bar somewhat thin, in a fore and aft direction, so as to be slightly flexible. Without this there is, I believe, some difficulty in ensuring that the weight shall bear well on *both* agate planes, so as not to run the risk of turning slightly about a vertical axis to and fro as it swings. I recollect someone (Sabine, I think), telling me that someone, I forget who, did not like the flexibility, and proposed to make the pendulum stiff, and Kater (I think it was) said, "He'll find it will not do."

The form having been chosen, we have to find the correction for the air experimentally. This demands the use of a vacuum apparatus. I think the most convenient plan would be to get a *fac-simile* of the pendulum made of wood. The resistance of the air depends only on the form and time of vibration of the pendulum, I mean supposing the state of the air given, and these would be the same for the actual pendulum and for the wooden model. By avoiding a specially dense wood we might easily get the model ten or twelve times as light as the actual pendulum, and the effect of the air on arc and time would be magnified ten or twelve times. The whole time of the swing would be reduced in the same proportion; but this would not signify as regards having a shorter interval by which to divide any error of observation of the initial or final coincidence, for the method of coincidences is so exact that it may be deemed perfect; that it is to say any error from this would be swallowed up by much larger errors from other sources; and that being the case there is a great saving of time in using the model, besides which we are less exposed to errors from variations in the clock's rate, changes of temperature, etc. However the actual pendulum might of course be used, and probably in any case an observation or two would be taken with this for controul. And besides the saving of time in taking the observations, resulting from using a wooden model, the possibility of taking swings at different pressures in close proximity, merely allowing an interval sufficient to allow the disturbance of temperature consequent on the exhaustion or admission of air to subside, would I think be conducive to accuracy as securing a more near identity in the rate of the clock on the occasion of the two swings that are to be compared.

You mention the corrections for pressure and temperature. The latter depends partly on the expansion of the metal, partly on the effect of temperature in altering the state of the air, and therewith the correction on account of the air. I am not sure whether or not you meant to include the effect of the expansion of the metal.

If it is intended to keep the two parts separate, I suppose it is meant to calculate the part due to the metal from the linear expansion either ascertained by direct observation or assumed as known for the kind of metal employed. As to the air, the correction for buoyancy, and that portion of the correction for inertia which would form the whole if there were no viscosity, both one and the other vary as the density, and therefore in a known manner as regards the temperature. The rest of the correction for inertia depends in a more complicated manner on the temperature. The whole of this residue for a sphere, and the first term and most important part of it for a not too narrow cylindrical rod, varies as $\sqrt{\mu\rho}$. ρ of course varies inversely as $1+\alpha\theta$ (θ the temp. α the co-efft. of expansion) but μ increases as the temperature rises, according to what law does not appear to be known for certain. I think experiments on transpiration gave it about as $(1+\alpha\theta)^{0.7}$, but I am away from books of reference, and I do not remember exactly.

If the temperature correction should be determined directly as a whole, *i.e.* effect on metal and on air together, by swinging the pendulum in air at two pretty widely separated temperatures, it is to be remembered that as it is made up of two different parts (effect on metal and effect on air) following different laws, the result will not be available unless some element (say the pressure) be kept constant. The experiment would involve the use of an apartment artificially heated in an equable manner,* unless we were content to wait all the time from one season to another, say summer to winter. The temperature correction so determined for a pressure of say 28 in. would not apply (on account of that part of it which depends on the air) to a pressure of say 3 in. It would seem to be best to correct as best may be for that part which is due to the air so as to get the part which is due to the expansion of the metal. I think the effect of the air can be got well by using a wooden model, and altering the observed effect in the ratio of Mh to $M'h'$, and M/M' can be got by weighing, and h/h' by balancing separately the model and actual pendulum on their edges.

I shall be happy to reply to further enquiries.

Yours very truly,

G. G. Stokes.

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* P.S.—With a wooden model the effect of the air is so much larger, the time of swinging so much shorter, and the expansion of the material by heat so much smaller, that there would be little difficulty in rigging up an apartment which would serve quite well enough for that.